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ABSTRACT

This paper examines concepts in information-processing theory which are likely to be relevant to development and characterizes the methods and data upon which the concepts are based. Among the concepts examined are those which have slight empirical grounds. Other concepts examined are those which seem to have empirical bases but which are misassessed. Included in the overview of information-processing concepts is an extensive look at J. Anderson's ACT model of basic memory processes. Other concepts from information-processing models which have been proposed to account for development include Sternberg's components of cognition model and Fisher and Pipp's skill theory. Finally, the general nature of modern methods is outlined, characterized, and contrasted with key aspects of the ethological approach. Appendices offer commentary on statements and concepts considered problematic in ACT, and a list of interpretive assumptions providing for a subject-determined integration of data. (RH)

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Information-Processing Theory and Perspectives on Development: A Look at Concepts and Methods --

The View of a Developmental Ethologist

by

Bradley Jesness

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Information-Processing Theory and Perspectives on Development:
A Look at Concepts and Methods --
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Section I
Preface and Introduction.

It is with some trepidation that I undertake the present endeavor. It apparently makes little sense to some to even examine the general merits and limitations of methods and associated concepts. While searching for a book on methods I spoke with fellow students of educational psychology who responded to my queries by asking what I was looking to investigate, and indicating that the topic of methods is a very broad one. (Regarding the latter point from a developmental or holistic standpoint, I'm not so sure.) Later, another psychology student who asked me the same question smiled broadly when I replied that my interest was methods and theory or (equivalently) theory and methods. She continued looking at me with her pleasant smile, so I added that it seems to be an area where one has little competition and that someone has to do it. While the latter statement is surely an overstatement, the question does arise as to when and after what kind of background and experience general questions tend to be posed. It seems that only those who have done numerous investigations using a particular approach take a close look at their methods and it is usually those in the twilight of their careers that attempt a thorough assessment. This is a questionable state of affairs.

Psychology and philosophy made a clean break from each other in the early part of the century. While this may well have been the best situation at the time, I believe it is time for psychologists to welcome back some philosophy of science into their programs. Some kinds of empirical methods are well established but there are

few indications that there is an appropriate on-going assessment of the methods and the related concepts. A few years back it was not uncommon for a psychology student to believe that "science is method." Today most would acknowledge the basic fact that science is theory and method, though most often how and why this is true does not come across as being clear or explicit. It is not uncommon for psychology students today to believe that theory, considered in general, is a set of beliefs which tie data together. And theory, they believe, can be anything you want as long as it parsimoniously and comprehensively subsumes the known "topic-related" data. Poppycock! As a student of other sciences I know that in general this is not what theory is. Predominantly, theory is rather a selective, precise account of key sequences (and patterns) of activities (and of the key aspects of the bodies (objects) involved). What many modern psychology students must be referring to is a collection of questions or hypotheses whose answers they have inferred, somehow associated with what data there is. It is a collection of directional hypotheses they hold dear as theory; they have no well developed theory because they have no body of clearly interrelated or directly related facts (i.e. no definitive data) on many of the topics of concern. That one must wait until he has expertise with a set of methods and in "theorizing" (as such) before questions can seriously be raised is preposterous.

Today in psychology there is a neglected methodology and, not unrelated, there is an entire relevant field of behavioral study that is neglected (and by many students unknown). The field is ethology. Researchers in this field study the behavior of other

animals using a systematic methodology and obtain reliable and strikingly meaningful results. Yet it seems to be of little interest to psychologists. Professors of psychology commonly have an ignorance of the methods, results, and perspective when these three aspects of the approach are considered together. They do not understand what ethology is and cannot assess the work when this is the case. Yet there is a new methodology there and nothing is so fundamental as a new methodology. Psychologists do not understand this and students do not study the work of these behavioral scientists. Students, not surprisingly, usually do not assess the work for themselves probably because of the situation with respect to the assessment of theory and method briefly alluded to above. Moreover, they may never be motivated to come to look at the work in ethology. The situation pains me, yet I don't expect it will change soon. Psychology is particularly liable to the socio-cultural context in which it operates. Some things in this science are especially likely to seem "self-evident", when in fact they are not. Principles and constructs of various sorts (today predominantly relating to cognitive concepts in attribution theory or information-processing theory) will seem most reasonable and statistics will seem to provide them with significance. But this latter fact has little to do with the long-range scientific merit of the concepts and related principles. The key to the long-range merit of ideas is the integrability of the data and whether the method of assessment clearly holds the likelihood for the continual refinement of concepts. In my view, this is the type of characteristics most of our data do not have and our methods do not provide. It is precisely these characteristics which the data generated by the ethological approach and the methods used in this approach provide. This is an important matter and something I will try to examine in outline form in the paper.

As a developmentalist I have been concerned with the ability of a system to adapt to the data and thus allow for an understanding of all types of things that may be involved in development. Given my views on the nature of the methods in ethology, it should be clear why a developmentalist might find this the methodology of choice. Students interested in immediate practical interventions may not immediately see gains from using this approach and may upon first exposure see no direct relevance of the data to their concerns. Indeed, in a way this is the case. An ethologist will try to let the subject explicate itself and will initially make no assumptions about particular interventions or manipulations that may have an effect. Until learning from the organism -- that is, learning about behavior in terms of other reliably associated (and generally, contiguous) behaviors -- no manipulations will be tested. And then only after some amount of such a subject definition may he experiment, and with what may be seen as very esoteric and slight manipulations. But, oh the power of a body of such data!

The purpose of this paper will be to look at some concepts in information-processing (i-p) theory which are likely relevant to development and to characterize the methods and data on which the concepts have their bases. Among the concepts examined will be those which have very little as their empirical bases. Other concepts described seem to have empirical bases which are misassessed, relating to situations where the concepts are over-generalized. Included in the view of i-p concepts will be a more extended look at J. Anderson's model. Then we will take a brief look at some other interesting concepts from other i-p models which have been proposed by their authors to be involved in accounting for development. The general nature of modern methods will then be outlined and charac-

terized and contrasted with key aspects of the ethological approach and methods.

Section II

Overview and Perspective on Information-Processing Conceptualizations

A. Anderson's ACT* Model.

Many see the ACT* model as a very detailed model and one that can be fruitfully applied to many areas to understand basic processes. In some cases this view seems well justified. ACT* is a very good model of some basic memory processes. Subjects presented with rather highly specified or standardized simple stimuli such as word pairs, patterns, or sentences will show predictable response patterns shortly later in time when "probed". Responses will differ predictably depending on the probe used at the test time and depending on such things as expectations, cues, priming, and thematic relevance.[1] The test probe effectively includes instructions which constrain the type of standardized response to be given. In such circumstances the ACT* approach obtains results which can be described mathematically and which are consistent in form and often in magnitudes to what's predicted by ACT* model.[2] These results cannot be ignored and other explanations of behavior must be consistent with these findings. It seems the basic memory processes modeled by ACT* would be involved in virtually all behavior.

But one must ask how much different various contents and capacities can differ from what's modeled by the ACT* system and still consistent with its findings? There is a basis from which to argue

that being consistent with the empirical findings may not be difficult. Kail and Bisanz, in discussing studies on cognitive strategies, note that coefficient of determination values will sometimes appear more impressive than they really are by "capitalizing on the variability in the data examined in a way that is psychologically uninteresting". They invoke this explanation to account for instances where any number of models might be used to explain the results of trivial experiments. A hypothetical example they use is an experiment which investigates solution latency for multiple-choice analogy problems with few vs. many alternatives.[3] This criticism may also be invoked in situations where the variability in responses is limited or constrained and especially when this fact is not addressed. In a way (in some notable cases) this criticism does not apply to the ACT* system (the model and approach). ACT* does seem to explain certain memory processes better than other models. Much of John Anderson's description and account of his work on basic memory processes demonstrates this and he documents the advantages of his system.[4] But the criticism (in its second formulation) does pertain to the application of ACT* to complex learning and problem solving situations. An example of such an application is the explanation for geometry problem solving protocols -- a rare case where Anderson has presented a description of how an ACT* "explanation" in such circumstances is tested.[5] (Other possible explanations of problem solving are cited in his 1983 monograph but not directly tested.[6] Some are not even computer-simulated.[7*]) When the model is applied to stan-

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* Computer simulations as tests of the model are liable to the same "constrained variability" criticism and are subject to another major criticism as well. Not only are responses constrained and inputs standardized but further direction is commonly given progressively as the simulation is in progress. This situation is too confounded to warrant further consideration in this paper.

dardized learning tasks with solution approaches which have been standardized by instruction, the usefulness of the ACT* "explanation" and of the ACT* model is exaggerated. Its application and apparent explanatory power likely result from capitalizing on the fact that the stimulus set and response set have a restricted range (and little variability of interest). ACT* researchers do not document or explain what is required for the givens in the immediate learning situation, but only for aspects, and still only aspects, of what transpires during bouts of short-term learning. It is notable that tasks have been appropriately selected for the age group and stimuli have been standardized over years of instruction. The foundations for selecting tasks and the principles involved in selecting tasks and the principles involved in determining the rate and sequencing of stimuli presentation indicate the lion's share of what the learning situation is. Correspondingly, much of the mental organization, expectation, and orientation of the subject are givens and moreover standardized. Are all the subject factors involved presumed to be "self-evident" and their pertinence obvious? These factors should not be forgotten and it should be noted again that ACT* does not account for them. Moreover, still only certain features of the subject's response protocol (as recorded) are likely predictable. Yet, the ACT* model (used loosely) is used to "explain" all short term acquisitions and facilitations of knowledge as <u>seems</u> consistent with empirically demonstrated basic memory processes.

One can thus readily ask what's not modeled. Before we try to answer this question first let's again look generally at some things that are modeled, with the aim of arriving at a better notion of lim-

itations to the kind of things accounted for. Given one begins with standard, easily communicable response-prompting stimuli and understandable instructions which dictate a standardized type of response soon after the administration of the stimuli, the model may be quite powerful. But even in such circumstances, what one might conceive of as the stimuli and "declarative knowledge base" will not always be such. Fortunately some adjustments can be made and the model can continue to apply. In fact, the adjustments and some subsequent changes can be seen in some ways furthering the application of the model. A good example is in reading comprehension. The comprehension units thought to be contained in a paragraph may not be processed as expected. Yet the i-p oriented practitioner may fall back on another declarative base, another set of stimuli rather easily specified given the standardized nature of the process. She may then strive to promote proceduralization with regard to such knowledge and develop the basic skills necessary for the original higher level standardized task.[8] Indeed her work is well justified. The simple but important process of proceduralization has been demonstrated and the stages of the process have been modeled and deomonstrated.[9] Surely this process is involved. Thus one's expectation that proceduralization can occur and then the next phase of learning can take place is well based. In general it may be that given that one can specify some standardized material which the subject deals with and some standardized goals and processes leading to some required response, the processes ACT* does model will seem notably among those which are taking place. This does not imply they are the most important processes, though they may be factors that will

be helpful for a practioner to keep in mind. In standardized situations where problem solving is involved it is not clear how much variability is accounted for by the basic memory processes of ACT*.

The original question of what's not modeled may now be usefully invoked in the problem solving context. Does ACT* have any empirical basis for being considered a model for higher order processes such as cognitive strategies? And more simply, are there any empirical grounds for considering ACT* a model for important processes occurring in situations where > 1 set of behaviors may be leading to > 1 set of responses or response patterns? This has not been demonstrated. In fact, a computer simulation of the basic model in such circumstances fails. More specifically, with propositions and pattern-matching characteristics preassigned and using standardized inputs, the model fails in simulation. Anderson reports "it was possible for a number of contradictory pattern nodes to reach maximum activation" and "partial matches could block the activation of more complete patterns."[10]

Still, the discussion above does not directly address the issue of what other factors, not formally modeled (consistently modeled) can operate in more complex circumstances. In other words, what other types of processes, however ill-defined, may be needed to address the problem of processing in many common problem solving situations. Many strange notions, with no directly demonstrated empirical foundations and not derived from the basic processes which have been investigated, have been proposed by Anderson himself. These are factors that seem necessary even though the basic processes are

likely overgeneralized and alluded to whenever possible. They are:

1. Some kind of constraints on the characteristics of propositions![11]

2. "Bottom-up activation", that is, activation based on situation specific stimulus characteristics (e.g. intensity). This activation it is said has characteristics similar to the activation of declarative memory [(which is more roughly "content dependent")].* This bottom-up activation can also involve inhibitory relationships [(i.e. situation specific vs "content specific" relationships)].[12]

3. Ubiquitous learning is called upon to explain activation from retrieval of elaborations, which is in addition to the activation from test probes, which is expected [(i.e. "modeled" -- loose sense)].[13] In other words, the ACT* system does not model many relationships among elements of declarative memory, though stimulus situations are conceived of in a way that will allow for such. "Past learning" is proposed as the source of the connections. The implication is that such past learning is inherently "arbitrary", dependent on whatever experiences the organism happened to be provided with or to chance upon. An ethologist has much reason to doubt this and to question lightly dismissing the problem. It is likely our human behavioral system contains the germs or seeds of what's necessary for developing knowledge and intelligence.

4. Goal structures with "goal dominance" and overriding the specificity and degree -of-match "principles" have been proposed. No information (evidence of any quality) is presented by Anderson on this matter.[14] It seems unwise to contradict basic findings.

5. A goal interpreter has been created so goal structures do not have to obey the data refractoriness "principle"![15] This is listed separately from the point above partly because it is so in Anderson's monograph. But it also implies something superordinate to goal structures.

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*I use brackets where I insert interpretations or comments before providing the reader with a citation.

6. Some productions are compiled as they are logically contiguous and not necessarily temporally contiguous.[16] (This notion could possibly be grouped with the third point or possibly with the first or last points -- who knows?) I should point out that there is no reason to object to the possibility of such; they simply have no bases in ACT*.

7. Pattern matching has executive functioning allowing partially matched productions to apply.[17] (This could be grouped with the last point or could be grouped with the third point, were it not for the supposed arbitrary nature of much learning.)

8. Last, but not least: New general procedures, whose strength does not depend on application and which are strengthened each time they would apply have been proposed.[18] Again, in principle I don't object. But, what are these? Are they the bases for stages or levels? If so, what kind of procedures are they and how do they manifest themselves?

All these proposed factors have no clear definition; no interrelated body of findings about their nature. And, they are not modeled consistently for they have no clear relationship to the basic memory processes. I have abstracted a number of additional problematic concepts from Anderson's "theory" and other problematic findings related in Anderson's monograph. I have grouped those above and these others under four major headings. These groupings may be found in Appendix I. Basically, the implications (inferences I draw) from the problematic notions can be summarized as follows: 1) Those regarding encoding: It appears there may be a lot to encoding which casts doubt on how easily the "declarative beginnings" of each bout of knowledge acquisition and processing can be identified. Many of the statements I have subsumed under this heading (in the Appendix) imply the bases of activation may be so subtle as to be more easily identified by productions. 2) Regarding internal representation:

There are many concepts and findings which imply the need for a large effective set of internal representations. To the extent that information is relevant cross-situationally, but is not all present in the present environmental context but is systematic, internal representation seems likely. On this basis internal representation is still the subject of a multitude of empirical questions. Until these questions are answered (systematically) internal representation may well be confounded with encoding and other processes. Since the mature researcher may presume much about the "presenting stimuli" and about elaborations, presumably learned, this is an especially relevant concern with regard to research on children. Moreover, we may find that encoding and internal representation have a dynamic relationship as "problem solving" progresses: internal representation would be cued; this might be followed by further cue coding resulting (contigently) in further internal representation, etc. (all possibly without any change in stimuli, overt behavior or any deliberation by the subject). 3) Many concepts and findings cannot be conveniently grouped as implying there's more to encoding or more to internal representation but seem to imply at least one or the other. 4) Finally, there are statements illustrating what might be termed "declarative bias" (see Appendix I). There are indications of things being explained, usually ad hoc, in terms of static "stimuli" with "objective/factual" characteristics whenever conceivable. Once the extent of internal representation is appreciated and its nature investigated we may find that the environment is to a great extent recreated internally as we interact with it. Things as such, it may well be that we model processes on that inner world as well

as assessing facts and processing facts and fact hierarchies. It seems a case may be made for how this would be both adaptive and efficient.

This concludes the examination of John Anderson's ACT* from a broad-minded (though, no doubt, a seemingly ungenerous) standpoint. One should note that further understanding of the nature, variety, and richness of processes may not be necessary for some limited practical interventions. Also ACT* will improve understanding whenever basic memory processes are involved. Yet, further understanding of other processes are likely important for accounting for complex processes and for development. Next we shall examine some other attempts to provide a comprehensive approach. These theorists explicitly apply their models broadly to "explain" the mechanisms of cognitive development. We will note what <u>else</u> is involved according to these psychologists. In the final section (Section III) we shall examine a new methodology that may be necessary not only for understanding of the richness of human cognitive processes, but to integrate findings (in general) and to assure continual refinement of data.

B. Other Information-Processing Models and Development

1. Aspects of Sternberg's Components of Cognition Model and His View of Development.

What follows will not be an attempt to detail Sternberg's model but just to highlight some features it has which Anderson's does not or which contrast with Anderson's ideas in ways which might be instructive. (Then a similar presentation of the features of Fischer and Pipp's Skill Theory will follow.)

Most notable in Sternberg's model is the centrality of metacomponents or executive components. These components are those which recognize a "problem" and select, sequence, and monitor performance and knowledge acquisition components. In short, they select, determine and control strategy. Moreover, one type of metacomponent (there are 9 types) "selects one or more mental representations upon which the other components and the strategy can operate."[19] No performance components (≈inductive reasoning processes) and no knowledge acquisition components (encoding, combining, and comparing) occur without providing feedback to the metacomponents. The importance of higher order thought and representation in learning for development is apparent. It seems there is little relationship between these processes and Anderson's basic "spread of activation" and "associative relevancy heuristic." More generally, Sternberg's "model" is distinctive from Anderson's notion of information processing. Anderson is much more reluctant to invoke things like metacomponents and keeps his explanations "close to the data".

Another notable aspect of Sternberg's views regards his notions of global and local processing and their relationship to what one might call the "declarative knowledge base" from which the organism can draw and upon which the organism can operate while learning in a given situation. These ideas seem to have implications for the nature of development. In new situations processing is said to be global with a limited general knowledge subsystem and a limited number of acquisition components applying. As learning and development proceed, local processing subsystems (≈Anderson's production systems) and even multiple local subsystems are developed. Local processing can then occur which is automatic and which can operate on a

very large knowledge base. In the local subsystems "metacomponents can instantiate themselves" and they do not differ from the other components. There is no hierarchical order among the types of components in local subsystems.[20]

It seems like internal representation of the environment and of other relevant circumstances would have to be very substantial for there to be no distinction between encoding, combining, and comparison components; inductive reasoning components; and strategy components. This also seems to have some significant implication for the nature of metacomponents, though the notion of no hierarchical order makes little sense to me in any case. One wonders how one might evaluate different contents and associated contexts for which all this could seemingly occur. Also, once this global to local processing transition has occurred it seems learning would slow as most aspects of development in an area would seemingly have run their course. But Sternberg says that when no local subsystems are able to satisfy their conditions the organism returns to global processing and a modified local subsystem will be formed. [21] In Sternberg's short essay pertaining to development it is not at all clear what this would mean in the organismic or environmental context. Would the impetus for change come from the organism or from new demands in the environment? If the impetus for such change comes only from the environment it seems likely that development would become slowed and gradual once commonly used local subsystems are developed. It is certainly unlikely that the data would reflect this, especially if what Sternberg considers local subsystems are formed early in life. In any case it seems obvious that components (including metacomponents) must undergo large qualitative changes during development.

Even though components are described only qualitatively it is surprising Sternberg does not say this.

When would local subsystems be developed? How often and in what situations would new local subsystems be developed? Many empirical questions remain, though what the specific questions should be cannot be discerned from Sternberg's model. Early on questions of levels of analysis would surely occur to many readers. The determination of sound bases for asking questions and the issue of levels of analysis will be addressed in the final section of this paper. Some evidence has been cited for the existence of the kinds of components of which Sternberg speaks (Sternberg, '80).[22] Still I'm inclined to agree with Gross who in writing about metacognition, points out that presentations relating the "discovery" of metacognitive components have a "show-and-tell" quality and their atheoretical nature is problematic.[23]

2. Aspects of Fischer and Pipp's Skill Theory

Fischer and Pipp's Skill theory provides an account of development which seems to recognize the need we may have to recycle our concepts en mass and at the same time explicitly give an important role to the organism in guiding the nature of its development. This Neo-Piagetian theory sees development as progressing through ten developmental levels (inherent somehow in the organism and/or its behavior). Each of these ten levels sets the <u>upper</u> limit on what develops for a time (until the next level emerges). This is interesting. Unlike Piagetian theory, an organism does not work to achieve a stage, rather it has a stage-like limit to its behavior at all times, which means that under certain optimal conditions the organism can demonstrate all stage-determined aspects at the <u>beginning</u> of

each stage. The stages, called levels, simply must instantiate themselves in more diverse and complex circumstances before the next level of ability (typically) emerges. In short, this is a notion of stage-determined behavior as "leading edges" in development. This seems to make a good deal of empirical sense given our knowledge of horizontal decalages. Unfortunately, Fischer and Pipp do not cite the nature of the initial and direct manifestations of these levels. All the evidence they cite is indirect.[24] Fischer talks about stages elsewhere as well and does so extensively, but again only cites indirect evidence and poorly formulated evidence.*[25]

In addition to accounting for horizontal decalages, the idea of a changing role of the organism in development is appealing for other reasons I will cite in the next Section. That any change in such a role would be powerful as it takes place in the adaptive complex is a likelihood. Fischer and Pipp describe a level as affecting all the basic transformation rules ("rewrite rules") -- the other basic developmental mechanisms.[26] Again, however, it is entirely unsatisfactory that the manifestations of the new levels as they emerge are not defined or indicated. The description of the effects of the new levels on the cognitive system correspondingly do not seem adequately clear to allow for a refined understanding.

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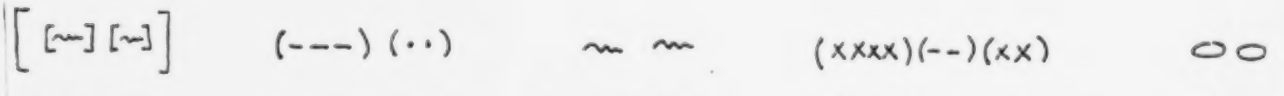
*Fischer "abstracts" information from the behavioral record in a way that is not scientifically acceptable (we will examine this problem in Section III).

Section III
The Ethological Approach to Studying Behavior

This final section is intended to bring into focus some very basic issues. Certain basic problems once recognized will surely make it clear why questions about the proper role of theory are so important and give us a clearer idea of what good evidence (data) might look like, in general.

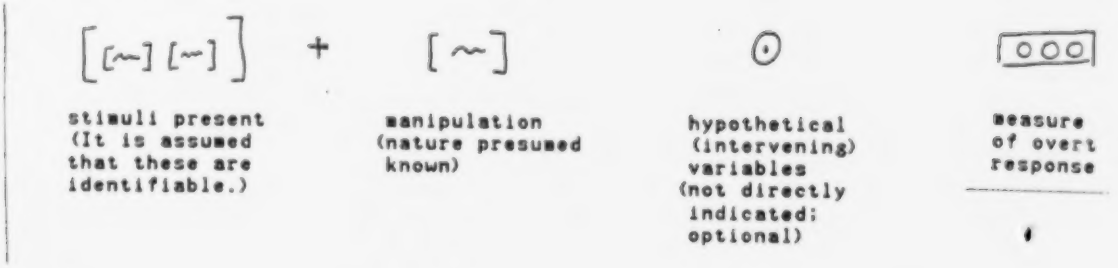
It will help to begin by putting things in a perspective with special heuristic properties. We will be trying to keep track of some very basic characteristics of research past and present and examine its pros and cons. To begin let's assume that enough research has been done to say some things with certainty: Stimuli must be processed to some extent before the organism responds overtly and/or before the stimuli act as information which will influence a later overt response. Experiments in psychology typically involve a measure of an overt response of some kind as a dependent measure and, though the independent variable may have its effect covertly, it is either measured indirectly or supplied as a stimulus. Given this state of affairs it is reasonable to illustrate the actual situation of the organism's response to a situation as follows:



the stimuli	initial encoding and processing	recognition and (usually) largely covert response	further processing (optional, but of interest in most psychological studies)	overt response

Compared with the actual behavior, what is it that S-R theorists and many attribution theorists measure? I believe this could

be summarized as follows:


stimuli present (It is assumed that these are identifiable.)
manipulation (nature presumed known)
hypothetical (intervening) variables (not directly indicated; optional)
measure of overt response

The associated theoretical system (methods and approach to interpretation) may be summarized as follows:

Problems:

1. When both overt and covert behaviors are considered, behaviors assessed in this approach are far from being continguous -- something that should generally require a relatively complex analysis (including many precise replications and extended studies).

2. The nature of the manipulation to the subject, though presumed, is not necessarily known and the unique aspects of the response (as compared with the control) are interpreted solely in terms of features of the manipulation (though "intervening variables" would be posited "when necessary").

3. Data such as this is highly discrete and must be integrated with other discrete data by relying on the intuitability of connextions (regardless of how "self-evident" the relationship of variables seems).

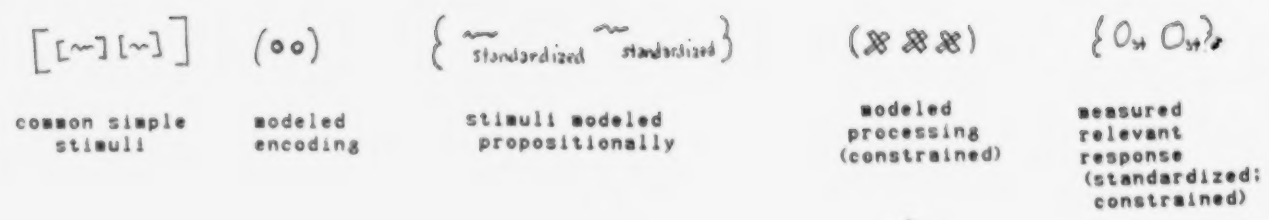
Major General (overall) Problem:

Separation of stimuli and response with <u>no</u> <u>indications</u> of connecting behaviors to verify conclusions or to aid in the integration of the discrete data. This is what the ethologist means when he says that while responses are overt, such are not elucidated by the other data and thus the data are not <u>objectively</u> <u>assessed</u> or <u>integrable</u>. (What is precisely meant will be defined soon.)

Major Advantage:

Behaviors are overt so limited interventions of practical utility may be clearly defined.

Many information-processing approaches may be characterized as follows:



Problems:

1. While more temporally contiguous behaviors of the organism are modeled, assessment of the characteristics of stimuli is ambiguous. The nature of presenting stimuli cannot be systematically related to other similar stimuli and objectively assessed as functionally similar stimuli. (Again, what is meant by objectively assessed, integrable data will be properly defined soon.) This problem is partly because the response is constrained and partly because the presenting stimuli is otherwise not objectively assessable.

2. While some basic processes can be measured indirectly and very reliably and their nature can be assessed, other processes may not be modeled.

3. Because of both points above and because of certain methodological preferences (acting effectively as limitations), the model cannot be directly extended to more complex analyses.

Major Disadvantage:

The nature of complex stimuli are not likely assessed in terms of relevant processes and responses are constrained. In short, the nature of the data as it pertains to complex behaviors still cannot be objectively assessed. The data is for these purposes discrete and one still relies on the intuitability of relationships between the data.

Major Advantage:

Some basic relatively contiguous processes can be assessed and related mathematically. It appears this can be done reliably with basic memory processes.

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Surely the reader's patience has waned and he is anxious to hear what an ethologist might consider objective data which is integrable. Hopefully, the following approach (heuristic) (related to the first diagram) will clarify such basics.: An ideal may be to be able to assess, or on empirical grounds, reliably infer, all ongoing behavior (overt and covert) and to have a notion of the stimuli such that, given the behavior observed, it may be related to other stimuli in similar circumstances. Indeed the reader may object and point out that this is often not the object of research. Rather, studying just selective aspects of behavior is surely oftentimes the object; a continuous record of continguous behaviors is not required. I wouldn't disagree, but at this point I will be trying to emphasize the need to be able to continually refine one's data and to be able to refine it in the light of new findings. For the time being bear with the artificial "ideal". Given equal ease of modeling, recognize that it is easier to accurately inter-relate contiguous behaviors. Furthermore, while not always necessary, it is crucial to have the ability to further assess behavioral patterning as needed. A more realistic and prudent approach will be outlined after some initial groundwork.

The reason one wants to be able to have an assessment of all of a contiguous set of behaviors is because such behaviors (in the situation observed -- considered broadly) relate, and without assessing their patterning one has no objective indication of their relationship (here's our definition of objective data).* At the same

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* Again, this writer recognizes that many of the behaviors and relations found would be trivial. When the actual approach is proposed this problem won't be present.

time (and related) is the necessity to have an objective (related) assessment of the stimuli in a situation. One will have such an objective, related assessment of the stimuli if the subject, in the segment of time observed and in the situation chosen, chooses his own stimulus set.

Surely this "ideal" which has been described in the last two paragraphs may sound like an impossibility and indeed to do anything close to this kind of assessment of any segment of a young child's or adult's behavior today would be impossible. Of course, it is only necessary to degrees, depending on the behaviors being investigated. What is crucial are the objective definitions and the ability to move towards such "ideals". One may move toward this kind of assessment in two ways. One is a direct approach and the other, an indirect supplemental approach. The direct approach is the essence or core of the new methodology alluded to early on in the paper. One will find that by itself, it is sufficient for the collection of data of the highest quality (objectivity), given only overt behavior is to be dealt with. Hopefully, once the method is described the reader will see that when used across subject matters and across age groups, one can make substantial progress toward an "ideal" (though again, the "ideal" may never be reached nor need it be). Hopefully the advantages of this for data assessment and likewise for the integrability of the data will soon be appreciated. The indirect approach will always be used only as an adjunct to the direct approach. Its use simply involves adding a few assumptions needed for relating behaviors at different points in development, but not seen (not overt) amongst the behaviors presently observed in very similar

circumstances (objectively defined). The indirect (supplemental) approach will amount to rules for inferring covert behaviors conservatively and at the same time leaving interpretation open to other possible factors.

The direct approach, the basics of which are now to be described, has been used reliably by animal ethologists for many years and with the result that data is continually refined and continually more integrable. The reader should notice that a high degree of observer competence (reliability) is implied. While this is highly unconventional, it seems necessary to assume this given the approach dictated by ethological theory and the research needs. To the extent to which this system has been applied to studying humans it has yielded reliable and surprisingly meaningful results. The indirect approach, to my knowledge, has not been used. The essence of the direct approach involves a strict definition of abstraction -- one used in order to properly (objectively) abstract data from observations. This definition applies not only to the direct but also to the indirect (supplemental) approach when used, as we shall see. It is also used to describe how the subject abstracts the information he may reliably use.* It provides us with a way of taking partial information without distortion so later other information in the same kind of circumstances may be gathered and thus our understanding refined. It effectively avoids all distortion (any distortion present will later be corrected with progressive research) and also discourages premature closure amongst our concepts. It is an account of actual regularities with no known approximation involved.

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* It is also used at all "levels of analysis" including the abstraction of stage-determined aspects of behavior.

It is directly related to observation (overt behavior and circumstances) when the direct approach is used; it is related to corresponding and related circumstances in the indirect approach, when it is used. It is effectively an exact description of aspects (subsets) of behaviors recorded in context -- both the environmental and behavioral context in which the behaviors occurred. As alluded to above, when the indirect approach is used abstraction includes a similar account of internal processing: these internal processes are related to behaviors always observed in younger subjects in similar circumstances (circumstances in part defined in terms of these operative behaviors). When possible these processes are assumed to differ -- present nature vs. past -- only because of internalization of contingent test manipulations and the corresponding circumstances and because of subsequent experience, where internal contents are altered solely in terms of simple associative and discriminative learning (given the other capacities present). The nature of other causes of change -- either what's assumed to be the cause or how it is termed -- will be dealt with very briefly soon.

Abstraction may most easily be further characterized in terms of what it is not. It is not a qualitative summary in any sense of some behaviors simply being "like others". Only actual similarities are appropriately abstracted and the similarities themselves are meaningless unless surrounded by similar patterns of behavior and the same environmental circumstances. What changes and what doesn't, in itself, does not matter; only in so much as a change is part of a reliable or constant pattern accompanying the same circum-

stances or when a change is part of a pattern reliably following a subject-determined change of circumstances is it considered significant. Finally, something is not said to preceed something else unless it does so exactly, given the highest standards of error variance in circumstance through which you care to view the subject.

Using the direct approach, the result of applying this definition of abstraction is needless to say a strict summary of overt behavior, where you have not acted to constrain behavior. Some readers probably feel that this is not enough and indeed the indirect approach can supplement the direct approach given one has similar data, i.e. reliable recordings of observations of younger subjects with somewhat different patterns in the same circumstances (and preferably, data at many intervening ages). But in this event a few assumptions are necessary and others are useful and will likely prove necessary. These, especially the latter which have not been previously discussed, will be described further, but first some words of caution. Blurton-Jones and the animal ethologists have gotten along very nicely without the indirect approach.[27,28] Moreover, it should be clear to anyone attempting an ethological approach to study and data analysis that a substantial number of direct approach studies must be done before the supplemental approach will seem appropriate. The indirect approach is a major area where researcher intuition must apply -- a researcher will have to decide when enough overt behavioral data has been collected, abstracted, and integrated to warrant such inferences. Fortunately, even if used prematurely, this will eventually become clear as more relevant (objectively simi-

lar) data is gathered. (One should note this is not true using other systems or theories we have discussed.) Nonetheless, while the ethological approach is noteworthy in its inherent tendency for "self-correction", such correction can be relatively long in coming with careless research interpretation. It is interesting to note that the only other place where observer (researcher) intuition enters the picture (enters the research process) is in deciding what overt behaviors, in what circumstances to study. The corresponding data gathered via the direct approach tends to bear fruit or be amenable to correction or redirection more easily than inferences made in using the indirect inferential approach. It should also be clear at this point that (as noted before) a complete record of behavior is not required (though more complete records may be found necessary). All that is required is the use of accurate assessments of behaviors and related assessments of stimuli AND the proper type of abstraction (exclusively).

Now the assumptions for the indirect approach: Recap of the "necessary" assumptions: 1) Again, as noted before, one must infer the simple internal representation or exact modeling of previously displayed overt manipulations and the accompanying circumstances. 2) One must assume behavior change is due solely to simple associative or discriminative learning and must use these simple processes to account for the overt behaviors displayed and the internal behaviors inferred unless one has no option but to infer otherwise. The type of first cause otherwise inferred and the way these and other factors are termed consistently within an ethological system or

theory is outlined in some detail elsewhere.[29] "Contingent" assumptions: Here, suffice it to say that it may be necessary that one periodically posits new emerging sets of behavior due to other causes (these include previously used behaviors now observed in new circumstances (without learning) or new patterns of previously observed behaviors now observed (without alterred circumstances)). The simplest possible causal explanation will be invoked and held until data forces citing other explicit environmentally related causes. This first cause to be presumed will be a new perceptual bias (assuming such has certain desirable properties within our system of thought). In fact it is not unlikely that these could be the causes of tremendous gradual and context-dependent shifts in overt behaviors as they emerge in the adaptive complex. One must recall that such a bias would emerge amongst other behaviors and amongst other behaviors related in a complex.

One might suspect that the above perceptual biases will affect the accuracy of the direct assessment method. Indeed occasionally this is very possible but in general such biases will have a more gradual effect leading to little inconsistencies among direct assessments. Given stages of development exist, perceptual biases will be assumed to be the cause unless we are compelled to cite other grosser, yet specific, changes in organismic responsiveness. (There is a way of consistently terming such other changes in an ethological model). The perceptual biases could seemingly have an effect similar to that attributed to emerging "levels" by Fischer. We therefore propose these as the causes of Fisher's levels. As previously noted, perceptual biases might have a gradual effect of shifting

numerous behaviors and in time have numerous manifestations. They are likely the type of thing that would most likely change both overt and covert responsiveness (they also fit well with the rest of the model). The way higher order processes might be initiated by such perceptual set biases is elaborated elsewhere.[30] Fischer himself might cite these as the initial manifestations of his levels if he had an appreciation for our definition of abstraction. Unfortunately, in speaking of stages he refers to "similarities" among individuals at each level and "differences" between levels in a way an ethologist would consider ill-defined.[31]

For a more complete list of assumptions of the ethological approach see Appendix II. A generalized account of the system for application to development in general and a more thorough presentation of assumptions may be found elsewhere.[32] There are a couple of things one might expect to find that if actually found would make the approach seem more likely to work. These should be discussed briefly: 1) Given one discovers stages (emerging gradually or otherwise) one might expect more recently developed behavior (behavior patterns) to be among those most overtly manifest. 2) Verbal protocols would likely include some aspects of behavior which have been most recently internalized. Protocols could <u>conceivably</u> give one something closer to a continuous record (so much as needed) but should be found congruent with inferences made using the indirect approach (only in this way will the self-correcting nature of the approach be guaranteed).

Conclusions about the ethological approach.

As simplistic and untraditional as the ethological approach may seem, it is merely the simplest and most parsimonious extension of the work of animal ethologists to human behavior. Noting a selected set of behaviors in selected circumstances (recall, both these activities are perfectly legitimate), using the strict definition of abstraction, and applying it to overt behavior, Blurton-Jones has made some startling discoveries. For example, in spite of commonly noted "similarities" in the behaviors, he has found no relationship between aggressiveness in the sense of striking others (outside of play) and rough behavior in "rough-and-tumble" play.[33] Also I believe he supports the sole use of 'abstraction' in the strict sense when he speaks of our inability to define "levels of analysis".[34] His position, as is mine, is that we are not competent to determine what behaviors are involved at a given "level of analysis" without a self-correcting approach <u>if</u> we expect the result to be a growing body of data, continually refinable and progressively integrable. The ethological approach will eventually allow for:

1. Definition of actual constraints on learning.
2. Evaluation of hierarchical structures and representations and mechanisms involved in goal interpretation, goal structuring and pattern matching.
3. Identification of new general procedures (emerging).

It is my view that the power of the ethological approach will be realized as soon as the data collected can, with some consistency and frequency, be related in ways which are non-intuitive and seemingly non-intuitable. This amount of data, I believe, would be gathered surprisingly soon after the approach was widely initiated.

Concluding Remarks

I bemoan the fact that what I've written is not likely being read by many who understand or have appreciated the power of the perspective as it's been used in animal investigations. Restraints on the length of this writing make it impossible to provide one with the flavor of some of the rich possibilities. Just as Anderson's theory can be seen falling short only after gathering numerous hints of shortcomings, so too the power of the ethological approach can be appreciated only by a listing of a number of unique findings about the adaptedness of various organisms. I heartily recommend Eibl-Eibesfeldt's Ethology, the Biology of Behavior and especially pages 1 - 215 and the last unit. This is sufficient to get a flavor. The power of the approach for human study can be detected in Ethological Studies of Child Behaviour edited by Blurton-Jones.

Interest in ethology, in spite of its probable relatedness to the study of human behavior, is not expanding rapidly. I hope that my efforts may help to further the application of the approach.

FOOTNOTES

[1]Anderson, John R., The Archictecture of Cognition (Cambridge, Mass.: Harvard University Press, 1983), pp. 86-121 ; pp. 171-204.

[2]Ibid. pp. 86-121; pp. 171-190.

[3]Kail, R.V. and Bisanz, J., "Cognitive Strategies," in Handbook of Research Methods in Human Memory and Cognition, ed. by C. Richard Puff (New York: Academic Press, 1982), p. 245.

[4]Anderson, J. R., op. cit. pp. 96-121.

[5]Anderson, J. R., The Architecture of Cognition (Cambridge, Mass.: Harvard University Press, 1983), pp. 217-225.

[6]Ibid. pp. 152-155.

[7]Ibid. pp. 157-162.

[8]Gagne', Ellen D., The Cognitive Psychology of School Learning (Boston: Little, Brown and Company, 1985), pp. 165-197.

[9]Anderson, J. R., The Architecture of Cognition (Cambridge: Harvard University Press, 1983), pp. 231-234.

[10]Ibid. pp. 147-148.

[11]Ibid. p. 60.

[12]Ibid. p. 149.

[13]Ibid. pp. 197-198.

[14]Anderson, J. R., The Architecture of Cognition (Cambridge: Harvard University Press, 1983), p. 136.

[15]Ibid. p. 161.

[16]Ibid. p. 238.

[17]Ibid. p. 32.

[18]Anderson, J. R., The Architecture of Cognition (Cambridge: Harvard University Press, 1983), p. 251.

[19]Sternberg, Robert J., "Mechanisms of Cognitive Development: A Componential Approach," in Mechanisms of Cognitive Development, ed. by Robert J. Sternberg (New York: W. H. Freeman and Company, 1984), pp 164-173.

[20]Ibid. pp. 173-176.

[21]Ibid. p. 175.

[22]Sternberg, Robert J., "Sketch of a Componential Subsystem of Human Intelligence," The Behavioral and Brain Sciences, 1980, 3, pp. 573-614.

[23]Gross, T.G., Cognitive Development, (Monterey, Calif.: Brooks/Cole Publ. Co., 1985) pp. 210-211.

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[25]Fischer, K. W. and Silvern, L., "Stages and Individual Differences in Cognitive Development," Annual Review of Psychology, 1985, 36, pp. 613-48, see pp 626-633.

[26]Fischer and Pipp, op. cit., pp. 60-68.

[27]Eibl-Eibesfeldt, Irenaus, Ethology, The Biology of Behavior, 2nd edition, (Chicago: Holt, Rinehart and Winston, Inc., 1975).

[28]Blurton-Jones, N., (ed.). Ethological Studies of Child Behaviour (Cambridge: Cambridge University Press, 1972).

[29]Jesness, B. L., Key Chapters and Sections of 'A Human Ethogram: Its Scientific Acceptability and Importance (in ERIC document collection, Nov. 1985).

[30]Ibid.

[31]Fischer and Silvern, op. cit., p. 641.

[32]Jesness, op. cit.

[33]Blurton-Jones. "Categories of Child-Child Interaction," in Ethological Studies of Child Behaviour, ed. by Blurton-Jones (Cambridge Univ. Press, 1972), pp. 104-112.

[34]Lamb, Michael, "Issues of Social Interaction: An Introduction," in Social Interaction Analysis, ed. by Lamb, Suomi, and Stephenson (Madison: The University of Wisconsin Press, 1979) p. 3.

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APPENDIX I

Problematic Statements and Concepts in ACT*

(Note to the Reader: All page citations refer to the location of the ideas in The Architecture of Cognition, J. Anderson (1983). Statements in brackets ([ ]) are my comments.)

Topic:
Encoding Below are statements, ideas, or concepts from Anderson's theory implying that there may be a lot to "encoding" and indicating a need for better understanding of encoding (casts doubt on how easily the declarative beginnings of knowledge acquisition can be identified). Bases for activation may be so subtle as to be more easily identified by associated productions or behaviors.

An ethologist would suspect much of "pattern matching" is simply adaptively tuned (innately discriminative) activation; i.e. activation may display a pattern across a subject's stimulus events to a much greater degree than might be expected. There may be less need for pattern matchers.

- Pattern matching takes time: pattern matching is a major temporary bottleneck. [(Pattern matching is not just active on "what's in WM." What's in WM takes time to develop.)] (p.33) (p. 88)

- In comparison to images and strings, propositional representations have stronger constraints. Only certain patterns are learned; humans have "learned to see certain patterns." (p.70)

- The conflict resolution principle (based on a set of 5 pattern matchers) is related to a theory of attention and perception. (Also, notably, it is related to problem solving -- see "Internal Representation" section.) (p. 126)

- Recent surprise: How much low-level processing affects high level processing (from expertise studies). (p. 128)

- Bottom-up activation: activation of nodes = activation of data [(situation dependent activation vs. content dependent activation)]. Moreover, it seems you can enhance this effect with progressive evidence [for what's already specified]. [(Progressive evidence≈progressive encoding and pattern matching?)] (p. 149)

- Also see the ninth point under "Internal Representation".

Topic:
Internal Representation Below are statements, ideas and concepts implying a need for a notion of large effective sets of internal representations (mainly, to account for sequencing).

An ethologist would expect that at present such sets would expect that at present such sets of representations are probably inextricably confounded with levels of encoding. Different encoding characteristics likely emerge developmentally. They emerge hierarchically and can be understood as perceptual biases. In the adaptive complex context in which they appear, they can result in powerful effects, including abstraction capabilities, and in appropriate sequencing.

- Higher order propositions develop from experience. The relations are more general than in specific circumstances (or events). [(Clearly one could infer huge sets of internally represented elements, all organized beyond the level of presenting stimuli. More than the associative relevancy heuristic is involved here; more than goals as conventionally described.)] (p. 75)

- Notably, the computer simulation of the model with propositional and pattern-matching characteristics assigned and applying the associative relevancy principle fails! (see p. 147: even with standardized materials the model cannot accurately predict the results) (p. 147)

- Anderson says that WM > STM momentarily. [(It is not clear to me that this has to be a momentary state.)] (p. 118)

- The conflict resolution principles (admittedly ambiguous) are related to a theory of problem solving and motor control (notably, they are also related to a theory of attention and perception -- see "Encoding"). (p. 126)

- Goal Dominance is more important than degree of match or specificity. "Goals help the idealization that pattern matching will function flawlessly."
  [(Indeed, in simulations such works well with simple standardized word matching, when E sets goals and subgoals. It works well with language acquisition when what a child has acquired determines what you enter into the simulation.) (p. 136)] (pp. 291-300)

- Current goals = a goal structure.
  (There is a lot to the "goal element".) (pp. 161-162)

- The "goal interpreter" restores the current goal so the data refractory principle does not apply. [(Here we have a controller of the guidance structure -- high level stuff.)] (p. 161)

- Planning (≈ anticipation) requires goal structures have to be able to be generated internally as well. [(Much organized mental activity occurs w/o environmental activity : This is often not modeled.)] (p. 167)

- [It seems possible that internal representation could result in environmental coding, resulting in further internal representation, resulting in more cue coding . . . This would certainly aid in maintenance of goal structures (and needless to say, this is relevant to the "Encoding" Issue).]

- There exist executive productions known as general problem solving procedures. (p. 219)

- Developed procedures no longer require declarative information be retrieved. [(Somehow it is directly activated or cued.)] (p. 235)

- Productions that are logically contiguous are compiled. Before compilation: "error checking procedures" and "inspecting the goal" occur. (p. 238)

- Production rules as : "implementing a search in which productions correspond to individual operators for searching the problem space" (p. 242)

Topic:
Other concepts related to encoding and/or internal representation, but not seeming more one or the other (includes some aspects of progressive retrieval).

To an ethologist there is a likelihood that such a class of ideas would exist. In our view modern psychologists confound levels of analyses. MOREOVER, useful levels of analysis cannot be determined without inter-comparisons of behaviors as they occur contiguously and in invariant patterns and sequences.

- Pattern matching includes executive functioning: "permits productions to apply even if their conditions are not completely matched." (p. 32)

- "The structure of abstract propositions is not a direct reflection of environmental structure. . . " Rather, it is an abstraction of an event. (Extraction process : "perceptual learning").
There is an ad hoc character to propositional representations because it is 'determined by experience' and not direct reflections. In general, the character of propositions is intuited (** see p. 71)

- Much activation can be w/o awareness. (p. 96)

- Successful pattern matching tests lead to more activation for a node. [(Problem: productions can provide info. but cannot directly energize nodes -- so something in such a statement is not specified)] (p. 146)

- "Bottom-up activation" (activation of nodes from "activation of data") is "similar to activation of the declarative network" and there are even bottom-up inhibiting relationships between nodes. [(It seems hard to tell bottom up (node activation) from top-down (declarative activation).] (p. 149)

- A goal structure is only partly conscious at any one time, the rest is retrieved. [(Progressive retrieval may be caused by production activity, but it seems that further encoding could be involved.)] (p. 161)

- SEE p. 175 : Retrieval of trace requires trace to be matched by a production that generates a memory report. [(What is caused by declarative activation and what's caused by this?)] (p. 175)

- Reverse fan effect (p. 179)

- Question : Recall: activation or generation?: Significant activation spreads from concepts not involved in the probe presented. (p. 189)

- Importance of context on recall : "Are there configural cues in memory?" (p. 200)

- In general : variable replacement (in productions; for generalization) should not take place unless constraints can be uncovered. (p. 244)

- "New general procedures" are special : There strengthening does not depend on actual application. Such productions are strengthened every time they would apply and such strengthenings apply to it and all its generalization as well. (? ! ; see p. 251)

Topic:
Indications of "Declarative" Bias (indications of bias to explain things in terms of static stimuli with "objective,factual" characteristics).

- "WM declarative" (p. 11)

- Activation : Associative relevance heuristic. [(Yet W/O executive functioning (pattern matching plus goals and/or general problem solving skills) this doesn't work.)] (p. 27)

- No inhibition process in declarative memory. [(May be inconsistent with inhibition in "bottom-up" processing influences cited.)] (p. 32)

- Initially knowledge "comes in" declarative form (static or merely dealt with interpretively via general problem solving productions). (p. 34)

- Regarding Anderson's criteria for adding new assumptions (pp. 42-44) :
  - w/r to #1 : implies (in its context) that pointing to ACT systems existence should be equated with centrality
  - w/r to #2 : relies on intuitability
  - w/r to #3 : under-played and under-used

- No partial memory for propositions [(not clear what this means because parts of propositions may be shared.).] (p. 52)

- Activation controls the rate of pattern matching and information processing. [(Apparently activation is not reaching its high point quickly even in a consistent environment. Pattern matching functions slowly presumably because productions keep firing and adding propositions. It seems likely that encoding may occur as well as further internal representations becoming active and the encoding may not be of what one would imagine.)] (p. 88)

- Outside of the current goal (a goal structure) "direct perception" sometimes keeps things active. [(I wouldn't presume to know what direct perception is -- esp. with a young child. Possibly one can guess closely when the activity is highly standardized.)] (p. 90)

- Waiting model for recognition [is biased "declaratively"] (p. 111)

- Special pattern matcher associated with "connectivity" said to be unique to propositions. (p. 116)

- [After all the pattern matching has occurred] the idea that the "goal element" sustains activation w/o rehearsal is declaratively biased. (p. 119) [(One can rehearse processes as well as facts)]

- Conflict resolution consumes capacity and conflict resolution schemes are simple. (p. 129)

- Higher level goals (i.e. goals moe general than immediate goals and their related structure) have no direct control, though can lead to setting of goals [(very mysterious)] (see footnote #5, p. 310)

- Individual clauses of productions correspond to bottom nodes in data flow networks. Pattern matching in ACT is a binary piecing together of such clauses. (p. 140)

- Anderson says the ACT system is always opportunitic in the sense that it is a response to a non-hierarchical control structure [Presumably he means it is always triggered environmentally by factors that have no inherent structure -- (VERY QUESTIONABLE)] (p. 162)

- Retrieval is of "declarative" information [(vs. generation of dynamic internal representation).] (p. 175)

- Elaborations which are learned are those retrieved in memory tests (Anderson's explanation for why activation comes from sources other than test probes.). (pp. 197-198)

- Compiled productions are always specific. [(Where did general problem solving procedures come from?)] (p. 236)

A P P E N D I X II

INTERPRETIVE ASSUMPTIONS PROVIDING FOR A SUBJECT-DETERMINED INTEGRATION OF DATA

I. Aspects of basic behaviors (and especially new and developing behaviors) can be observed in "conflict situations". Conflicts between species-typical behaviors in need of further integration result in varying degrees of "debilitating stress".

II. All behavior must be viewed with the past history of the subject in mind. This is necessary to understand what behaviors and patterns are new and what behaviors have been intercoordinated or conditioned and which have been "internalized".

III. Covert behavior (internalized behavior) can be inferred by an observer who has engaged in extensive longitudinal study, which involved naturalistic, unobtrusive observation and notation of species-typical behaviors.

Internalized behavior can save a subject many steps in problem solving. Such behaviors would be inferred to be the same as when last overt, except for the numerous opportunities to be inter-coordinated in situations where they may have been cued. Inference of the nature of covert behavior in relation to overt behaviors presently observed can take this possible further coordination into account, since one has noted such opportunities as arose for intercoordination.

IV. Only unobtrusive observations used to interpret the behavior of single subjects will be unbiased, be subjects considered as members of groups or as isolated individuals.

V. All behavior is directly or indirectly interactive with the environment and all behavior is homeostatic or works towards homeostasis. Moreover, all species-typical behavioral developments leading toward mature functioning are adaptive, not only with respect to the environment, but with respect to the subject's other behaviors.

Definition of Principle of Homestasis (as it applies to psychology): All basic behavior (species-typical behavior) either returns the organism to some steady state or is directly instrumental towards establishing the next level or degree of adaptation.

VI. For behavior to be viewed properly, it must be congruent with present evidence and with inferences about the subject's perceptual-intellectual skills (i.e. all behavior "must be construed within the subject's perceptual-thought system").

VII. After I - VI, understandings of behaviors are appropriately integrated. Interpretation is made by comparing present and past behaviors. Interpretation is made in terms of associative and discriminative learning, internalization and intercocordination, and (possibly) the emergence of a new behavior pattern. The latter evidence themselves when groups of behaviors alter in responsiveness and shift together in responsiveness due (presumably) to the emergence of a new perceptual bias (or, possibly a set of perceptual biases).

The utility of the above assumptions gains in likelihood if one believes development can only be understood as a series of stages (whether continous or discontinuous). Also if one reflects, it seems likely that the most recently developed and least integrated and last intercoordinated behaviors will be most overtly manifest. Continuous verbal reports accompanying overt behavior will also likely most reflect the behaviors most recently internalized and may well help provide something close to a continuous record of behavior. Some knowledge of development nonetheless will probably be necessary.

The powerful effect of "simple" perceptual biases as they emerge in an adpative complex should certainly be considered and appropriately explored.